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THE sudden and premature death of CLIFFORD DOBELL on 23 December 1949, in London, deprived the scientific world of one of the outstanding protozoologists of all time—a man who had made important and lasting contributions to biology, medicine and the history of science.

Clifford Dobell (though christened Cecil Clifford, he never used the first of these names) was born on 22 February 1886 at Birkenhead, in Cheshire. He was the eldest son and the second of the five children of William Blount Dobell (1859–1927), and his wife Agnes *née* Thornely (1852–1942).

The Dobells are an ancient English family, probably descended from Angles who settled in Kent and Sussex before the Norman Conquest. Their name is in Domesday Book but its derivation is not certainly known; in the older records it is spelt variously, but since about 1600 the present form has been usual in the main branch of the family, to which Clifford belonged.

Clifford's father, William Blount, was left motherless when only three years old. At the age of seventeen, when his father emigrated to America with his second family, he went to Birkenhead, where he worked in the office of the Lancashire Coal Company for a few years, until he set up as a coal merchant on his own account. In his twenty-fourth year (1883) he married Agnes Thornely, who also early became an orphan and was brought up by her grandfather, Samuel Thornely of Liverpool, and a spinster aunt, Caroline Thornely. Agnes had a good education for a girl of her generation—at school she learnt German, French and some Italian, and had excellent instruction in music. She was very musical indeed, and could play the piano, the violin and the organ well enough to make her wish to take up music professionally after leaving school: but her grandfather would not hear of it. Clifford's love and appreciation of music were undoubtedly inherited from his mother: his father was quite unmusical.

On both sides of his family there were relatives distinguished for ability in a variety of ways. Sir Basil Mott, Bt., C.B., F.R.S., the famous civil engineer, at one time President of the Institution of Civil Engineers, was a cousin of Clifford's father. The well-known architect, Sir Arnold Thornely, was his mother's cousin. Among his more distant cousins was a poet, Sydney Dobell. Sydney's sister, Mary Alice, married Briton Riviere, R.A. Other women in his family also married distinguished men: Constance Mott, a second cousin on his mother's side, married Dr James Alsop, a founder and sometime Pro-Chancellor of the University of Liverpool: it was she who endowed the Alsop Chair of Music there. The wife of his old friend, Sir David Bruce, F.R.S.,

was a more distant relative. His own sister, Hope, married Professor P. G. H. Boswell, F.R.S.

In his early childhood Clifford was sent to a kindergarten school, but after a few terms there he was withdrawn and put in the charge of a governess at home. When he was six years old, the family left Birkenhead and settled in Llandebie, in South Wales, where his father had bought a colliery. The children keenly appreciated the change, for the new surroundings were beautiful, and they had all sorts of animals to amuse them, including a pony to ride and drive. Young Clifford also took a great interest in the wild flowers and birds in which the country abounded. He could read at an early age and devoured any book he could get hold of. He was extremely clever with his hands and spent much time in the workshop learning carpentry, a craft of which he made full use later in life. Life in the country, however, was not so idyllic as it might have been, for, as in many middle-class Victorian families, the young Dobells were very strictly—even severely—brought up, and suffered much from being denied the freedom of less sheltered children. But their stay in pleasant rural surroundings did not last long, for after four years in South Wales things went wrong at the colliery and the family had to move to Southport, where they lived for some years in rooms. Here young Clifford took French lessons and also went to the Southport School of Art, where he attended classes in drawing and painting, working with great enthusiasm in and out of school hours. At that time he was determined to become an artist—and above all a painter of animals: his paintings were shown to his distant relative, Briton Riviere, R.A., who praised the work but commented that there was no money to be made in art.

When Clifford was thirteen his parents decided once more to live in the country and they went to Great Sutton, Cheshire, leaving him behind in Southport as a boarder at Sandringham School, where Mr R. A. Chadwick was headmaster. Until then he had been taught only by an inefficient governess, and knew nothing of systematic lessons, apart from those at the School of Art; nevertheless, his general knowledge, picked up from books, was wide and varied for his age. At first he had some difficulty in adapting himself to school life, which he disliked intensely; but his extraordinarily retentive memory and strong will standing him in good stead, he soon had no difficulty in mastering all the subjects that were taught and could always keep his place at the top of his class. He was fortunate in getting at Sandringham School an excellent grounding in the classics and in acquiring a real taste for the Greek and Latin languages that he never lost. More by natural bent, one supposes, he became a great admirer of Shakespeare, and learnt *The Rubáiyat* of Omar Khayyám from beginning to end, so that he could quote from memory many passages from these and other poets. He also became interested in comparative religion and refused to be confirmed, thereby asserting the agnostic attitude which he retained for the rest of his life. By the age of sixteen he was more and more eager to adopt a scientific career, in which resolve he was encouraged and aided by Mr Chadwick, who had a very high opinion of his pupil's abilities and personally prepared him for his entrance examination to Cambridge University.

He entered Trinity College in 1903, at the unusually early age of seventeen. He always considered it a stroke of good fortune to have had for his tutor there the distinguished zoologist, Adam Sedgwick, whose inspiration and friendship had a great influence upon his development and ultimately determined the course of his scientific career. Passionately fond of animals, he thought at first he would like to be a veterinary surgeon, but he soon changed his mind and took up medicine instead, passing in 1904 his First, and in 1906 his Second M.B. Examinations, with distinction in human anatomy and physiology. In 1905 he gained an Exhibition in Natural Sciences, and in 1906 got a Major Scholarship and took the Degree of B.A. with first-class honours in the Natural Sciences Tripos, offering—in addition to the two medical subjects—zoology and chemistry, and winning the highest marks for the whole university. In 1910 he obtained the Degree of M.A. As an undergraduate Dobell had a very difficult time financially. In fact, his father would not have been able to send him to Cambridge if Mr Chadwick had not generously come to the assistance of his favourite pupil. Much later, during the 1914–1918 war, Dobell learnt that the money which his father had borrowed was not repaid, and he promptly settled the debt in full—plus compound interest—from his hard-earned savings.

After graduating in 1906, he had resolved—with Sedgwick's approval and encouragement—to abandon the study of medicine and devote his life to protozoology, a subject in which he had by then become deeply interested. He received his early training from W. S. Perrin, who subsequently became a well-known surgeon, but in those days was University Demonstrator in zoology under Sedgwick, then head of the Zoological Laboratory. Sedgwick long cherished the hope of founding a department of protozoology at Cambridge: and so, when Perrin had graduated with distinction and desired to take up research, Sedgwick persuaded him to study the Protozoa, and sent him for training to Prowazek—one of Schaudinn's most zealous disciples — under whom he worked in Rovigno (Istria). In his reminiscences of Perrin, Dobell (1936a) thus recalls their relations: 'I first got to know Perrin properly in the summer of 1906. . . . I was put to work beside him in the old Zoological Laboratory, with instructions to pick up as much as I could. . . . As we were engaged in entirely different investigations, and both eager to learn all we could, the distinction between master and pupil soon vanished. . . . Sedgwick's dream of a department devoted to Protozoology had momentarily come true! It took shape as one small and dingy room, divided accurately down the middle, with Perrin as high priest in the right half and myself as novice in the left.'

The two continued to work side by side during 1906 and 1907, living on their scholarships and by coaching in the evenings. 'These were great days for both of us', continues Dobell: 'We worked in our laboratory and in our rooms in college for seven days in every week, and for anything up to sixteen or seventeen hours a day. . . . Looking back on these early days, I now realize how lucky I was to have had Perrin for my mentor. . . . He it was who taught me many of those little tricks of the trade which are seemingly so obvious but really so baffling to a beginner.' During this period Dobell was attending advanced

courses of instruction in zoology (under J. J. Lister), physiology, biochemistry and bacteriology (under G. H. F. Nuttall). He also took a leading part in the Cambridge Natural Sciences Club, where he read a number of papers on subjects so diverse as parthenogenesis, phrenology, the future of man, and death: he also held office, first as its secretary and then as its president.

When Dobell was a young graduate, Germany was still the Mecca to which biologists aspiring to extend and perfect their knowledge made pilgrimage. So, like others, he was advised to see what the University of Munich could do for a protozoologist, and at the end of 1907 he went there to study at the Zoological Institute under Richard von Hertwig. While his association with Hertwig himself and with Franz Doflein left the most pleasant memories, he was utterly repelled by the speculative views and slapdash methods that he found current among the majority of protozoologists he met—disciples of Fritz Schaudinn, who, though just dead, seemed even more of an oracle than when living. Dobell found the prevalent uncritical attitude so unsympathetic that he was very unhappy during his six months' stay in Germany, and his experiences there at an impressionable age influenced his feelings towards the whole country for the rest of his life. German culture remained as antipathetic to him as that of the Japanese was congenial.

He afterwards went to Naples where he worked at the Zoological Station until June 1908 and collected most of the material for his famous study of the life-cycle of the coccidium *Aggregata*.

On his return to Cambridge he was elected a Fellow of Trinity College (1908–1914), and in the same year he won the Rolleston Prize (Oxford and Cambridge) and the Walsingham Medal (Cambridge)—both awarded for original research in biology; in addition he obtained the Balfour Studentship of Cambridge University (1908–1909). As Balfour Student, in the middle of 1909 he travelled (in the company of R. C. Punnett) to Ceylon, where he stayed several months, conducting various researches on parasitic protozoa in the laboratories attached to the Museum at Colombo and to the Botanic Gardens in Peradeniya, as well as in the field. In Colombo he was the guest of A. Willey, F.R.S., then Director of the Museum, from whom he received much assistance in his work. Soon after his return to England, Dobell left Cambridge to take up an appointment at the Imperial College of Science and Technology in London, first as Lecturer and ultimately as Assistant Professor of Protistology and Cytology (1910–1919), under his old master and friend, Adam Sedgwick, who for a time occupied the Chair of Zoology at the College. Here, in addition to his immediate duties of teaching undergraduates and supervising the research work of graduate students, he continued his own protistological investigations. In 1913 he again visited the Mediterranean, this time working at the Musée Océanographique in Monaco, where he collected more material for his study of *Aggregata*. During his stay in Monaco he became friendly with Dr M. Oxner who was in charge of the Museum.

Those who knew Dobell at the Imperial College speak very highly of him as a teacher. His lectures were remarkable for their scope and clarity and for the

beauty of the blackboard illustrations. In the laboratory he was always ready to help in any problems of research, but to the young student his manner at first seemed brusque and he was thought to be too exacting. A meticulous and polished worker himself, he expected the same high standard from others and was openly contemptuous of second-rate performance. Nevertheless, although students in his class had to exert themselves far more than with the other lecturers, they later expressed their indebtedness to his rigorous inculcation of scientific method.

During the First World War, under a scheme initiated by the Medical Research Committee and the Royal Society, the Imperial College released Dobell for service with the War Office, to conduct protozoological investigations of dysentery cases and carriers among the troops, and to train biologists in the diagnosis of intestinal protozoal infections. He was engaged in this work from the end of 1915 to 1919, at the Wellcome Bureau of Scientific Research, which was placed at the disposal of the War Office for this purpose. From this period he devoted himself almost entirely to researches on the intestinal protozoa of man, and soon became one of the foremost world authorities on this group of organisms. By that time he had already published most of his papers on general protistology, and such was the excellence of this work that in 1918 he was elected to the Royal Society—at the age of thirty-two he was one of the youngest Fellows.

In 1919 he resigned from the College when invited to accept the post of Protistologist to the Medical Research Committee (later Council) at the National Institute for Medical Research, which thenceforth was to be the scene of his activities until the end of his life. Though at the College he had performed his duties with his usual competence and was regarded as a first-class lecturer and teacher, he himself was not satisfied with the choice he had made, and after Sedgwick's death—in 1913—he realized that he did not fit into academic life. He once wrote: 'I was not cut out for a University Professor. I am not serious enough on some subjects and far too serious on others.' In fact, his extreme individualism prevented him from collaborating with others, while his intolerance permitted no compromise. The post at the National Institute seemed to offer a chance to earn his living as a scientist in the way that suited him best, for there he would not have to do any teaching but would be free to devote himself entirely to research work. Before taking up the new appointment, Dobell discussed the programme of his researches with Sir Walter Morley Fletcher, the then Secretary of the Medical Research Committee, who fully approved it and assured him of his support. Moreover, on the staff of the Institute there were a number of workers whom he knew well and liked; therefore he had no doubt that he would find there a congenial atmosphere, and he accepted the offer enthusiastically. In one of his letters of that period he wrote: 'I shall be free to do what I like, when I like. It seems too good to be true.'

The thirty years spent at the National Institute for Medical Research were devoted almost entirely to researches on Protozoa of medical importance.

During that period he also completed and published his paper on the life-cycle of *Aggregata* upon which he had been engaged for sixteen years. In this connexion he paid a tribute to the Council's 'enlightened concept' of *medical research*, which had permitted him 'to pursue his zoological studies in freedom' (Dobell, 1925*a*). He also found time to make a number of contributions to the history of protozoology, including his historical masterpiece *Antony van Leeuwenhoek and his 'Little Animals'*.

Meanwhile, the auspicious conditions under which his work at Hampstead began had gradually deteriorated. As the personnel changed in the course of years, those of his colleagues who—like Sir Patrick Laidlaw, F.R.S., and Captain S. R. Douglas, F.R.S.—understood him and his aims, had died or resigned. Moreover, the development of modern science increasingly involved team-work, and the growing dependence of scientific investigation on outside control caused emphasis to be laid on research that promised results which could be immediately applied to the solution of 'practical problems'. This short-term policy exasperated a man who had spent years in the patient study and elucidation of some of the fundamental problems of medical protozoology. He complained that his work was not appreciated by those with whom his lot was thrown, for they could not understand that 'science must be made before it can be applied' and that 'no man can serve two masters' (Dobell 1936*b*). To an individualist like Dobell these conditions became peculiarly oppressive, for he could not make adjustment to an environment that he considered to be hostile to his work and the ideals that inspired it.

As the years passed, his reaction became more exaggerated and he continued to work in an isolation which did not bring him satisfaction and deprived his colleagues of much that he might have contributed to the common cause, if only he had been willing to compromise. He was happy while lost in his work, for he was a whole-hearted devotee of science: but he was more sociable than most people supposed, and one of his consolations in those days was meeting fellow-protozoologists from outside the Institute who visited him in his laboratory. With them he would 'talk shop' for hours, and as his knowledge was profound and his talk vivacious and amusing, they always left with a feeling of having spent a profitable and entertaining time. In 1942 he decided to take the Sc.D. of Cambridge, a degree which he could have obtained any time during the preceding twenty-five years if he had so wished. His reasons for taking this belated—and for him perhaps unnecessary—step are characteristic of his mood at that time. In a letter that he then wrote to one of us he said, 'I have no desire to hear myself called "Doctor". I have no vanity or feeling of any sort about such things, and am quite content—if not desirous—to go on being called "Mister". . . . I took this degree at last purely for what I may call comprehensively "business reasons". I regard it as an investment, and think it will be of use in the very uncertain days which are before us.' But, though he felt that the atmosphere of the Institute had become unsympathetic, it was still the atmosphere of a laboratory; and, since he could work in no other environment, he clung desperately to the familiar scene. During the last years of his

life, as he saw the fateful sixties approaching, his despondency increased and a tragic note appeared in his letters to friends, for there remained so much of his life-programme to accomplish, and though he felt himself still young, the time for action was growing short. In June 1949 he wrote: 'I am now told definitely that the Institute expects to move out to Mill Hill in November, and that thereafter I shall have no facilities for continuing my present work. So I have just five months in which to finish off the work of a lifetime (and discard for ever what can't be finished). My last year of office is to be spent in "writing up my results", so that everything is cleared up by the time I reach the age of sixty-five, when I shall be unemployed and theoretically unemployable'. It was the prospect of being 'theoretically unemployable' that really troubled him now. He knew it was inevitable that he should retire at sixty-five, but without even a small laboratory such as he had occupied while salaried, there was no possibility at all of carrying on research during his years of living on a pension: and without the company of his 'animalcules', life seemed quite blank. In September of that year his wife took some photographs of him in the old laboratory which he had occupied for thirty years. Commenting on one of these (the frontispiece to this article) he wrote to one of us on 6 December 1949: 'I can't believe that it shows me at the end of my days as a protozoologist —despite the fact that everything indicates that my time is nearly up'. These words—in one of the last letters he ever wrote—proved to be prophetic, for on 10 December Dobell had a cerebral haemorrhage, after which he lingered on in a semi-conscious condition for a fortnight, dying on 23 December 1949. He was thus mercifully spared the empty years he so much dreaded.

We now turn to Dobell's scientific work. His precocity was remarkable, for by the age of thirty he had published all but one of his general protistological papers. Most scientists have some cause to be apologetic for their *juvenilia*, but even Dobell's early work is singularly flawless and mature. His paper on *Copromonas* (1908*a*), written when he was only twenty-two, put him at once among the protozoologists who were going to count. In this work he demonstrated the whole life-cycle of a new flagellate, including its sexual phase. Since syngamy has been conclusively established for only one or two other holozoic forms in the heterogeneous class of Mastigophora, this observation in itself justified the sub-title of the paper, 'A contribution to our knowledge of the Flagellata'. In a paper on the intestinal protozoa of frogs and toads (1909*b*), he dealt with the less spectacular organisms—chiefly flagellates—which had received scant attention from zoologists since Ehrenberg described them as best he could sixty years earlier. He also gave the first adequate account of *Entamoeba ranarum*, the study of which served as a good introduction to his future work on *E. histolytica*, the causative organism of amoebic dysentery in man, which is morphologically indistinguishable from the parasite of the Amphibia. In his precise study of all these protozoa, the cytologist never gets the better of the naturalist, who sees their structural details, and makes the reader see them, as organs in living creatures. The labour involved in sorting

out the different elements of a rich mixed fauna and flora in the amphibian gut has been stressed by the author himself: 'It is necessary to study . . . not only the organisms, but also the cell-remains and other debris. Only by conscientious adherence to this slow and tedious method can satisfactory results be obtained. It is a pity that this elementary and obvious precaution has been so frequently neglected.' It is a precaution that throughout life he schooled himself to take. Improvements in staining technique since 1909 have revealed in the trichomonads structures that escaped even Dobell's keen eye, and he erroneously attributed to these flagellates an encysted stage, but in essentials his early work has stood the test of time.

These two early publications have been instanced as models of the true naturalist's approach to investigations demanding observational accuracy and freedom from prejudice, combined with what may be called fellow-feeling. One would have to go back a century or two to find the inhabitants of the microcosm appreciated and revealed in this way. There is no doubt that Dobell was from the first a traditionalist, deriving much of his outlook from sympathy with the minds and methods of the early microscopists whom he revered so much. Traditionalism may also have had something to do with his scorn for the fashion of the moment. But he also possessed an innate and fiercely independent critical faculty, which had undoubtedly been fostered at Cambridge by his association with the robust and sceptical intelligence of Adam Sedgwick.

Dobell's natural tendency to accept nothing on hearsay and his passion for testing things for himself had been intensified on his return from Germany (*v. supra*), when he gave vent to his feelings by publishing a number of papers in which he showed in no measured terms his disapproval of the speculative approach to protistology. Of these the most ingenious in its argument is 'Chromidia and the binuclearity hypotheses' (1909*c*). Though its interest to the biologist is now largely historical, the spirit with which Dobell attacked humbug and confused thinking should never go out of fashion.

In 1911 he published a number of papers on the bacteria. One of these, 'Contributions to the cytology of the Bacteria', he himself regarded as important among his earlier work. For many years bacteriology was entrusted to workers to whom—in Dobell's words—'the Bacteria are but a means to an end—they study them in order to cure a cold or make a cheese'. Bacteriologists took little interest in the finer structure of the microorganisms. In his cytological studies Dobell opened up an approach to bacteriology which has since been followed in many directions. Moreover, as we shall see, his early familiarity with the organization and habits of bacteria proved to be of great value in connexion with his later investigations on the cultivation of human intestinal protozoa.

In the same year there appeared his 'Principles of protistology', which made game of the 'cell theory' in so far as this regards the protist as the equivalent of a single cell of a metazoan or metaphytan organism. In this paper he pointed out that, between them, the theory of evolution and the cell theory have had a paralyzing effect on protistology, since 'they have forced men to see the

Protista from an entirely subjective point of view'. 'The cell theory was a great generalization, but a generalization that was purely verbal.' Dobell admitted that he was not the first to criticize this theory, but to him belongs the credit of providing an alternative interpretation. He believed that the Protista are organized on quite a different principle from that of other organisms. The arguments he used are so cogent that his thesis is now generally accepted among biologists: even elementary textbooks nowadays are at pains to explain that the Protozoa should be described as 'non-cellular' rather than as 'unicellular'. The logical conclusion from this assumption was that the Protista could no longer be regarded as 'simple, lower, or primitive', and that there is no justification for a phylogenetic system that derives the multicellular organisms from the Protista as we know them. Ten years later (Dobell & O'Connor 1921) he maintained that the Protozoa should rank as a sub-kingdom of the animal kingdom and that the four great classes into which they are usually divided should be raised to the status of phyla. He also believed that these micro-organisms were 'likely to yield very valuable information regarding many obscure biological phenomena', and in a letter to one of us (1918) he said: 'Biology is now about where physics and chemistry were in the time of Bacon and Newton: and it is going to come out of the ditch in the same way'.

The interest aroused by the 'Principles' can be judged by the fact that it was translated into Russian and Italian. There is evidence that Dobell intended to deal with this subject more extensively, for among his papers there is an unfinished manuscript of a book entitled *The elements of protistology: an introduction to the study of non-cellular organisms*. The text of only the first part, which is an amplification of his 'Principles', is available, but the scope of the second part is indicated in the table of contents.

Of Dobell's papers appearing between 1912 and 1915, the majority deal with the bacteria and with protistological genetics. Among the protozoological papers of this period there is one on amoebae (1914*b*) that provides an object-lesson on how to set about the study and classification of these amorphous microorganisms, where only nuclear structure, peculiarities of mitosis and encystment provide reliable criteria to 'specific' characters.

There can be no doubt that his most important contribution to general protozoology is the monograph on *Aggregata eberthi* (1925*a*) upon the study of which he was engaged intermittently for sixteen years from 1908 onwards. This paper deals with the life-history and chromosome cycle of a coccidium living alternately in two hosts—cuttle fish and crab. He elaborates certain points in the cytology which he thinks may be of general application within the group of Telosporidian Protozoa and which support his previous contention that, in nuclear features as well as in their body-architecture, the Protozoa stand quite apart from multicellular animals. He considers that the nucleus of *Aggregata* should be described as 'a nuclear system', for in every adult it is essentially binucleate, one of its nuclei lying within the karyosome of the other. He believes that this nuclear structure is characteristic of the Coccidia in general. He further demonstrates conclusively the haploid nature of *Aggregata* at every

stage of its development except the zygote, which alone is diploid—and for a brief period only, since its first nuclear division is meiotic. Already in 1915—with one of his pupils, A. P. Jameson—he had predicted that both gregarines and coccidia would prove to be haploid organisms. While subsequent cytological studies of gregarines by other workers are somewhat conflicting, there is no doubt that in all the coccidia so far investigated from this point of view the reduction division is zygotic, as shown by Dobell for *Aggregata*. Since both sexual and asexual stages of this parasite are haploid, and have apparently identical chromosomal constitution despite their morphological and physiological diversity, he challenged the views held by orthodox geneticists regarding the significance of the chromosomes in inheritance: 'For those who believe, from a study of the Metazoa and Metaphyta, that the chromosomes—any or all of them—"determine" morphological characters, "bear" hereditary properties, or constitute "the physical basis of heredity", the foregoing facts should afford ample matter for reflexion' (*loc. cit.*).

We shall now consider Dobell's contributions to medical protozoology, which were concerned exclusively with the intestinal parasites and especially the amoebae. The stimulus to the new orientation of his work was provided by the First World War, when he devoted all his energies to the study of amoebic dysentery. Though by 1914 the causation of this disease was known (largely as the result of the classical researches of the American investigator, E. L. Walker), this knowledge was not general, and few among those who were called upon to investigate the disease in the early part of the war were familiar with the facts or able to recognize and differentiate the parasites in the human host. At that time it was known that, in addition to the dysentery parasite, *Entamoeba histolytica*, man also harboured a non-pathogenic amoeba, *E. coli*, but during the war three more harmless amoebae were found in man, of which one, *Dientamoeba fragilis*, was discovered by Dobell with Jepps (1918*b*) and another, *Iodamoeba bütschlii*, was first accurately described and named by him (1919*b*). One of the important facts which emerged from the work carried out by Dobell, and by various protozoologists working under his general direction on behalf of the War Office, was the discovery that a considerable proportion —estimated at 10 per cent—of the world population, including that of Great Britain, were healthy carriers of the 'dysentery amoeba'. He elucidated the significance of such carriers in the aetiology of amoebiasis, and of the host-parasite relations in this disease, pointing out that only harmonious relations between man and the amoeba permit the latter to complete its biological cycle and propagate its species in new human hosts. In the course of this work, Dobell demonstrated that a series of cases, each examined only once, disclosed only about one-third of the infections present, and he was the first to emphasize the importance of repeated faecal examinations in arriving at a correct estimate of the incidence of intestinal protozoal infections.

In addition to the protozoological aspects of amoebiasis, he carried out (alone and with others) investigations on the action of drugs in this disease, and he played a leading part in improving the method of its treatment with

emetine bismuth iodide (E.B.I.), which was adopted as a standard by the War Office Dysentery Committee—of which he was a member—and by the Ministry of Pensions. The results of the protozoological investigations carried out during the 1914–1918 war by Dobell himself, or under his direction, are incorporated in a number of special reports issued by the Medical Research Committee (later Council) (1917–1921). As no epidemiological investigations of amoebiasis have subsequently been carried out on a comparable scale, while the position regarding this disease has remained virtually unchanged, these reports still serve as an important source of information to all medical men and parasitologists interested in the subject.

The vast knowledge and experience of the human intestinal protozoa which Dobell gained during the war was consolidated in the form of two books. One of these, *The Amoebae living in Man* (1919), is—as the sub-title indicates—'A zoological monograph', embodying a careful revision of all the contemporary knowledge of these parasites, which are fully described and shown in illustrations executed with the author's usual skill. This monograph is justly regarded as a classic: though written thirty years ago, it is still an indispensable guide for all serious students of protozoology and tropical medicine. The other book, *The intestinal Protozoa of Man* (1921), was originally conceived as a joint work with F. W. O'Connor—whose name it also bears—but was actually written entirely by Dobell. It is a valuable practical handbook on these parasites for medical men and also for zoologists, containing accurate accounts of all the known species, and dealing with the diagnosis and treatment of the protozoal infections. The value of both these books is enhanced by the fact that they were based largely on the author's own original researches on this group of parasites, on which by that time he was a recognized authority.

During that period he also became interested in the human coccidia. It is now known that man may be infected with coccidia of the genus *Isospora*, but in the years between 1915 and 1926 it was thought that he might also harbour coccidia of the genus *Eimeria*, of which three species were described, one by Wenyon and two by Dobell. However, in 1926 Thomson & Robertson demonstrated that these *Eimeria* were merely coccidia of fish. Although Dobell (1919*a*, 1921*a*) was not the first to attribute eimeriid parasites to man, and the last-named investigators had—in common with other parasitologists—themselves previously accepted the same view, the entire blame for the misinterpretation was put on him and some critics made of this episode an occasion for sneers as unseemly as they were unjust. This was, perhaps, the only serious slip he ever made, but he erred in good company.

The discovery by Boeck & Drbohlav in 1925 of the first reliable method for the cultivation of the intestinal protozoa found an immediate response from Dobell, who saw that it afforded the means of elucidating *in vitro* the complete life-histories of the amoebae living in man, as well as of studying their bionomics and response to drugs. He first set out (alone, and in collaboration with Patrick Laidlaw and Ann Bishop) to improve the culture media, devising new ones and modifying the old ones, until he had at his disposal a number that were

reliable for a variety of purposes. These media served for tests of the effect of emetine on *Entamoeba histolytica in vitro* which showed that the drug had a direct lethal action on the amoebae, but did not influence them indirectly, through the host, as was formerly assumed. The exact minimum dose required to kill the amoebae was determined only after an all-fluid medium was elaborated, in which the drug was not absorbed by the solid component present in the earlier media. New light was thrown on factors influencing the life-cycle of *E. histolytica* in culture, the knowledge of which facilitated the study of its viability at different stages and under various conditions, and made possible the induction, at will, of its encystment and of its hatching.

Dobell also studied the intestinal protozoa of macaque monkeys, which proved to be morphologically indistinguishable from those found in man. He realized that these hosts offered a promising and as yet unexplored field for the experimental investigation of many unsolved problems concerning the human intestinal protozoa. Though more than half a century had elapsed since the discovery of the dysentery amoeba (*Entamoeba histolytica*) and its harmless congener (*E. coli*), and about a quarter of a century since the minor intestinal amoebae of man (*Endolimax*, *Iodamoeba* and *Dientamoeba*) were put on the map, the knowledge of their life-history had hitherto been limited to two stages, the trophic amoeba and its cyst: nothing certain was known about the stages of development intervening between the mature cyst (the infective form) and the adult amoeba which is found in every infection. Dobell was convinced that these 'serious gaps in our knowledge of these organisms . . . could be filled in by means of properly planned and carefully conducted experiments', using cultures for this purpose, and that the study of the host-parasite relations in human amoebiasis could be carried out with monkeys, which 'harbour a varied protozoal fauna remarkably similar to that characteristic of human beings; while monkeys as a group . . . are sufficiently akin to us to warrant the assumption . . . that what is generally true of their protozoal inhabitants is probably also true of ours'. This assumption was fully justified by subsequent investigations in which he demonstrated experimentally the identity and interchangeability of the intestinal protozoa of man and macaques (1931*a*, 1933, 1934, 1935, 1936*c*). From study of the natural infections of *Macacus* with the *histolytica*-like entamoeba, he demonstrated that in all its structural and cultural characters this organism is identical with the dysentery amoeba of man. He found that it was possible to isolate any strain from man or from monkey and maintain it in cultures indefinitely; and that he could obtain any stage in its life-history and infect with it a fresh simian host, as well as eradicate the infection at will by therapeutic means. Host and parasite could therefore be separated or united as he saw fit, and any desired experiment could be made with either or with both combined. No such power of control had yet been attained with any other endoparasite of man or other animal.

The results had been gained by rigorous and unremitting attention to detail, both in the cultural methods used for the entamoeba outside the body of its host and in the care given to the health and habits of the experimental animals.

The vigour and contentment of Dobell's monkeys became famous among his visitors. He kept all his monkeys as personal pets, and actually gave more attention to their diet, health and well-being than he gave to his own. Work of such perfection could hardly be delegated to assistants: for years it had entailed upon him an almost invariable seven-day week, and he was able to take only rare holidays for short periods. The outcome, however, was a series of remarkable investigations, published over fifteen years (1928–1943) under the general title *Researches on the intestinal Protozoa of monkeys and man*, and representing a body of new and secure knowledge of permanent value to biology and medicine.

In the first—and most important—paper of this series (1928), he gave a fully illustrated description of the life-history of *E. histolytica*, as observed in cultures. Using new methods, he succeeded in obtaining its complete development outside the human body and observing for the first time all stages in the hatching of the cysts and the subsequent transformation of the hatched organisms into ordinary amoebae again. For this part of the life-cycle he proposed the term *metacystic* development. Ten years later (1938*a*) he published a detailed account of the life-cycle of *E. coli*, which was a pendant to the paper on *E. histolytica*, its frequent companion in the human bowel. The exact account of the comparative development of these closely related and medically important parasites had long been a desideratum of medical protozoology. With the publication of these two papers the complete life-cycles of the two most important intestinal amoebae of man were fully elucidated. The next organism to be studied in cultures by Dobell (1940*b*) was *Dientamoeba fragilis*. Though of no obvious importance to the physician, to the general protozoologist this curious binucleate organism proved to be the most interesting of all the intestinal Protozoa of man. A careful study of its cytology and development showed that the binucleate condition commonly seen in this amoeba actually represents an arrested telophase stage of mitosis, the apparently granular structure of the nuclei resolving itself into a constant number of chromosomes. Furthermore, it became evident to Dobell that *Dientamoeba* was not a true amoeba but an aberrant flagellate closely related to *Histomonas*, the protozoon causing the disease known as Blackhead in turkeys, from which it differs in the absence of a flagellated stage. The last published paper in this series (1943) was devoted to the cytology and life-cycle of *Endolimax nana*: it contained new observations on the mitosis and the first account of the metacystic development. In concluding this paper, Dobell wrote: 'Of the five species of amoebae which live in the human intestine, I have now worked out the complete life-histories of four', adding (in a footnote): 'The fifth—*Iodamoeba*—is still under investigation, and I hope to describe its life-history later'.

That he had already made considerable progress in this direction is evident from the *Report of the Medical Research Council* for 1939–1945, and from his letters to us. In one of these (10/11/44) he states that he has studied all the division stages of *Iodamoeba*, but has not yet drawn them; and in another (11/1/47) he says: 'So far as I know, I am the only person who has hitherto

succeeded in cultivating *Iodamoeba* (free from other species) continuously. It is very difficult to isolate, but when isolated grows in all the usual media for *E. histolytica*; it does not encyst *in vitro* in ordinary media.' He also added that he had not studied the excystation and metacystic development of this amoeba. Only a few months before his death, however, he told us that, after working on it for some years, he had triumphantly completed his investigation into its life-history and so, with it, the whole series of researches on the intestinal amoebae of man, adding: 'It is going to surprise you a lot'. However, after his death, not a trace of a manuscript or notes on *Iodamoeba* could be found among his orderly papers, although the daily records of observations and the corresponding slides were all there. Dobell always maintained that, throughout his professional writing, he never went in for rough drafts. 'I think it all out first, to the minutest detail', he once wrote: 'and then—as it were—after this thorough digestion and mastication and rumination, regurgitate it slowly and painfully'. His method seems to have been to rely up to the last moment on the extraordinary retentiveness and precision of his memory. The whole story of *Iodamoeba*, which was stored in his brain, thus died with him.

When Dobell started his researches on the intestinal amoebae, he discovered that bacteria had an important influence upon the growth of these organisms in culture, and he soon turned his attention to the study of this question. Already in some of his earlier works (1926–1931) he had shown that some species of bacteria were favourable for the growth of amoebae, while others were harmful. He also found that certain species may have a determining effect upon the life-history of *Entamoeba histolytica*, and by manipulating the bacterial flora he was able to induce cyst-formation. He continued his investigations on this problem up to the time of his death, and in the *Report of the Medical Research Council* for 1945–1948 it was announced that 'He has now found it possible to grow the amoebae [*E. histolytica*] in pure mixed culture with one or more known species of bacteria (instead of in the usual way with a complex and unknown mixture of bacteria), and so to ascertain the part played by each species and its effects on the development of the amoebae. Many such pure strains, of known and accurately determined bacterial composition, have now been made and studied in detail. An outcome of this work has been the preparation of pure cultures of the amoebae themselves—unaccompanied by any other living organisms—and methods of obtaining and propagating these are being further investigated.' This work was soon completed, and he was engaged in making a record of it right up to his last days. After his death, a manuscript was found of a paper representing the twelfth instalment of the *Researches on the intestinal Protozoa* entitled 'Bacterial factors influencing the life-history of *Entamoeba histolytica* in cultures'. Though provided with a full table of contents, and even bearing one of his aptly chosen mottoes, this manuscript was unfortunately not completed. However, with the help of the index and the daily records which he had kept for many years, it has been possible to reconstruct the missing parts and to prepare for publication Dobell's swan-song, his last contribution to a subject to which he devoted the best years of his life.

Though the last instalment of the *Researches* is still unpublished and another (on *Iodamoeba*) is irreparably lost, Dobell practically fulfilled the mission which he undertook more than thirty years ago, namely to work out the complete life-histories of the amoebae living in the intestine of man. The contemporary state of knowledge on these organisms was clearly and exhaustively set forth in his monograph (1919*b*), while most of the unknown facts regarding their course of development and bionomics were subsequently discovered by Dobell himself and described in the series of researches referred to above. Among the questions which interested him was the pathogenicity of *Entamoeba histolytica*. In the monograph he expressed his strong conviction that it was an obligatory tissue parasite, invariably producing intestinal lesions in man; but his subsequent work on cultures of this amoeba and on its behaviour in monkeys caused him to change his opinion and to admit that it was capable of leading a commensal existence.

These researches are a lasting monument to one of the finest protozoologists of this or any time. They can be characterized in his own words (1919*b*) as 'the work of a biologist and not of a medical man—of one whose chief interest is, and has ever been, in the Protozoa themselves and not in the diseases which they produce . . .'. There can be no doubt, however, that his researches were also of fundamental importance in the study of the protozoal diseases, and that he himself never lost sight of the practical bearing of his investigations. Thus, he wrote (1931*a*): 'To the casual onlooker it may seem absurd to devote years of intense effort to the solution of a problem which apparently possesses merely systematic and academic interest'. But to this he adds: 'Since *E. histolytica* is . . . a cause of painful disease in Man, it is obviously in his own interest to learn everything possible about this organism. . . . Consequently, we want to know—and know certainly—whether the several *histolytica*-like species of *Entamoeba* frequently found in other animals . . . are, or are not, of the same species as our own parasite. We want to know this not only for the sake of advancing zoological knowledge, but also for our own protection: we want accurate information regarding the host distribution of *E. histolytica* in nature, because such knowledge is not merely of academic interest but also of practical personal importance.'

We cannot dismiss the account of Dobell's scientific work without referring again to its quality. Most of his researches during the last quarter of a century were carried out concurrently, and it took years before he was satisfied that the results were ready for publication. His experiments, which were performed with meticulous care and precision, are described in the greatest detail. Some would say that his accounts of the work are unnecessarily detailed; however, since they leave nothing unsaid and anticipate every possible criticism, a future investigator studying the same problem could confidently take Dobell's last paper on any aspect of it as his starting point, without having to wade through the papers by other predecessors. We have Dobell's own testimony to the almost superhuman labour entailed in this work. In one place (1934) he writes:

'My plan has been, throughout, to make a minimum of experiments, but to make each with the maximum of accuracy. Every experiment and observation has been made with my own hands and eyes: I have had no assistants, and rely upon the testimony of no "technicians".' In another place (1938*a*) he says: 'There is no short cut to knowledge of the Protozoa—even for a man of genius. The goal can only be attained by accurate observation, carefully controlled experiment and unremitting work. For such studies "genius" is, perhaps, even a disqualification.' And further: ' "Many things nasty a-doing look nice when done." Not in arrogance or pride, but because they seem apposite, I have set these words of an ancient poet at the head of this memoir. . . . Though Ovid, like my slow and laborious methods of research, seems no longer in the fashion, his verse conveys something true to me: and it also expresses something of the momentary satisfaction which I now feel in recording the results of a tediously protracted piece of work—albeit my satisfaction is tempered by still vivid remembrance of the almost intolerable drudgery and disappointments often entailed in its performance.' To his critics he answered in the words of Leeuwenhoek: 'In these observations I have spent a lot more time than many people would believe; yet I made them with pleasure, and paid no attention to those who say to me "Why take so much pains?" and "What's the use of it?": because I don't write for such folks, but only for Philosophers.'

Dobell's work on the intestinal protozoa of man may serve as a model of the scientific method in action. In his papers the student can see how an experimental problem should be approached, and its investigation planned, executed and reported. Moreover, as he reads, the student comes to realize that a scientific paper need not be written in the jargon of the textbooks: for most it is a revelation that a work may be erudite and yet elegant—even witty—in presentation. Since 'le style est l'homme même', it was to be expected that when Dobell wrote, he would express himself in a direct, sensitive yet virile, English, never pedantic or affected but inevitably revealing the man of culture and scholarship. The artist in him found partial expression in the use of words. He was fortunate in his editors, who accepted—even in the days of acute paper shortage—mottoes and quotations from the classics that the author considered vital to the development of his thesis. These well-chosen literary fragments reflect the high ideals which inspired his work and scientific outlook. The high quality of his papers and books was further enhanced by the exquisite and accurate illustrations, the value of which he himself did not underestimate, when he wrote (1943): 'They are, I think, perhaps more objective records of my observations than my words. To me it has always been easier to draw correctly than to write correctly.' He derived great pleasure from the execution of his drawings, which were portraits rather than diagrams and presented an outlet for the artistic side of his personality.

An appreciation of Dobell's scientific work would not be complete without mention of his printed criticisms of the work of others. These could be severe and outspoken, his caustic remarks often wounding their victims more than he himself fully realized, and so making him enemies. However, his method of

disputation was all part of his intense and uncompromising regard for truth and accuracy in scientific research. Any departure from his own high standard in these essentials, such as shallow thinking and avoidable errors of observation, was to him anathema, and he felt it his duty to say so. On the other hand, he promptly recognized the merits of a good piece of work and always gave it unstinted praise. His outlook could not be better stated than in the words of Leeuwenhoek, which Dobell has already applied to himself (1932): 'As I aim at nothing but Truth and, so far as in me lieth, to point out Mistakes that may have crept into certain Matters; I hope that in so doing those I chance to censure will not take it ill: and if they would expose any errors in my own Discoveries, I'd esteem it a Service: all the more, because 'twould thereby give me Encouragement towards the Attaining of a nicer Accuracy'.

In addition to his original investigations of the Protista, in which Dobell was professionally engaged, he had always showed a deep interest in the history of protozoology and bacteriology, and to this he devoted most of his spare time, reading and writing at home: 'mostly at the dead of night . . . after a hard day's work at my own research, and with another similar day in the laboratory before me'. His knowledge of the history of his own branch of science and of the personalities engaged in it was extraordinary. There can be no doubt that, among his contemporaries, he was the greatest authority on the history of protistology.

For our appreciation of Dobell's historical work we have been fortunate in having the assistance of Professor F. J. Cole, F.R.S., whose notes have been freely drawn upon in writing the following lines. Dobell's interest were first directed into the historical channel in 1909, when he was studying the intestinal Protozoa of the Amphibia. He was surprised and greatly interested to find that they had already been seen and described by Leeuwenhoek in 1683 and 1702. He then began to study Leeuwenhoek systematically, and discovered that he had described some thirty-five species of Protozoa in sufficient detail to enable most of them to be identified by the specialist. In 1915 he drew attention to the fact that the Dutch microscopist had in 1683 described a case of Mendelian dominance in the rabbit, and, in 1920, he established beyond question that Leeuwenhoek was the first, in 1681, to observe intestinal protozoa in man, and had left a description from which it was possible to identify the organism as the flagellate *Giardia intestinalis*. In 1922 he found that, as early as 1674, Leeuwenhoek had described the oocysts of the coccidium *Eimeria stiedae* in the rabbit. Dobell maintained that these were 'the first microscopic observations on the Protozoa, the first of any parasitic protozoon, and the first of any species of the Coccidia'. In 1931 he published an important bibliographical paper, in which he announced that thirty-two of the earliest of Leeuwenhoek's letters, which had not been included in the Dutch and Latin collected editions of his works, were not lost, as had been generally assumed, but were practically intact in the Archives of the Royal Society. All this material has been incorporated in the complete correspondence of Leeuwenhoek, now being published in Holland by the Leeuwenhoek Commission.

Dobell's studies of the work of the Dutch microscopist culminated in the publication of the monograph *Antony van Leeuwenhoek and his 'Little Animals'* (1932), in which, as the sub-title says, the author gives an 'account of the Father of Protozoology and Bacteriology and his multifarious discoveries in these disciplines, collected, translated and edited from his printed works, unpublished manuscripts, and contemporary records'. This scholarly publication was a masterpiece. It is Dobell's most important historical work and through it his name will always be associated with that of his revered spiritual father. Even to say that this great work occupied his 'spare' time for more than twenty years does not adequately express the amount of labour he put into it. He had great energy and powers of intense application, with which much can be accomplished, especially when—as in his case—the daily routine is not regarded as completed before 3 a.m. However, as with his other achievements, assiduity and determination do not alone account for the completion of this monumental treatise. He was ready to sacrifice his leisure and he possessed the necessary learning, linguistic equipment and historical insight demanded by the task. Leeuwenhoek is a baffling author to comprehend and translate. His language is the obsolete Dutch of the seventeenth century, while his style is eminently personal and colloquial. He eschews scientific terms and employs instead homely expressions and similes, not found in any dictionaries. To the translator and commentator 'it was a task', to use Dobell's own words, 'nearly as great as that which faced the first readers of the Rosetta Stone, and it was accomplished by similar methods', for Dobell was able to bring to it the faculty of historical divination, a subtle and intuitive intelligence, and, last but not least, a vast knowledge of protistology. 'I imagine that I know the subject-matter of his writing on protozoa and bacteria as no mere scholar or philologist can ever hope to know it', he commented (1932). All these qualities enabled him to give to the world a fascinating, original and living work, which has already taken its place among the famous classics of its kind. Referring to the part played by it in the revival of interest in the works of Leeuwenhoek, Dr W. H. van Seters (1950) writes: 'we should never forget that it was Dobell, an Englishman, who prepared this "renaissance" and made it possible'. In the course of these studies, Dobell made a unique collection of notes, cuttings, pamphlets, etc., pertaining to Leeuwenhoek. In accordance with his desire, this valuable material, the *Leeuwenhoekiana*, has been presented by his widow to the Royal Society.

Among Dobell's historical papers, one which all scientific historians and workers should read and inwardly digest is his 'obituary' notice of 'Dr O. Uplavici (1887–1938)' published in 1938, recording the history of the misinterpretation and perpetuation of the Czech title of a paper 'O úplavici' (=On dysentery) as the name of an author, 'Dr O. Uplavici'. This amusing but instructive satire was rightly thought worthy of reproduction in *Isis* by Dr George Sarton. In 1939 Dobell cleared up the confusion which had developed in the synonymy of *Trichomonas tenax*, the only flagellate normally living in the human mouth, and, in a paper on 'monstrous Latin parasites' he registered an emphatic warning against the ignorance displayed by many zoologists when

coining generic and specific names. In 1947 his historical studies acquired a new direction when he detected in a Scottish bookseller's catalogue, and purchased, a manuscript volume entitled *Kincardine Papers: Letters from Robert Murray to Alexander Bruce*. This collection comprised 116 letters written in English between 1657 and 1673 by Sir Robert Moray, who was the first President of the Royal Society before the Charter. Dobell (1947*a*) traced the devious history of this important volume and presented it to the library of the Society, which, as he remarked, was the obvious resting place of the transcripts after all their adventures.

Dobell's literary gift found fullest expression in his biographical work. In addition to Leeuwenhoek's life-history described in the monograph, he wrote a series of obituary notices, containing his appreciation of the life and work of biologists whose point of view or personality attracted or inspired him. The most outstanding of these is the masterly article on his oldest friend, Sir D'Arcy Thompson (1949), which was also the last paper published during his life. His obituary and commemorative articles are as meticulously documented as his strictly scientific papers, and usually disclose details not commonly known. The remaining ones are devoted to the following persons: T. R. Lewis, L. Joblot, C. G. Ehrenberg, B. Grassi, W. S. Perrin, R. v. Hertwig, V. Lambl, M. Siedlecki, F. Vejdovsky, J. Drbohlav, H. S. Jennings, C. A. Kofoid and W. Bulloch. He could have written many more biographical sketches, for stored in his memory was an amazing collection of personal information and anecdotes about living and dead protistologists, with which he liked to entertain his friends. However, the publication of some of the startling revelations he made might have been actionable under the law of libel. Closely connected with his historical work was his passionate interest in books, which he collected with love, expert knowledge and with money which he could sometimes ill afford.

We have already noted that most of Dobell's historical work was done in the night. He first developed this habit at Cambridge, when preparing for his final examinations. He discovered that he really needed only five hours' sleep, and after that he regularly devoted the early part of the night till 3 a.m. to reading, writing and studies outside his professional duties. He maintained that the normal healthy man does not require more than four or five hours' sleep; but this was a degree of asceticism to which few could aspire with impunity. He proved, however, that he himself could 'take it': throughout most of his life he remained astonishingly healthy and he retained his youthful vigour up to the last. Most of us thought that he would live to a very great age.

Dobell's scientific connexions and interests can be judged from the following list of British and foreign learned societies to which he belonged (the year of admission is given in brackets). He was a Fellow of the Royal Society (1918), of the Cambridge Philosophical Society (1907); Member of the Marine Biological Association (1910), Fellow of the Royal Society of Tropical Medicine & Hygiene (1917), of the Zoological Society (1919), of the Royal Society of

Medicine (1920); Foreign Fellow of the Reale Accademia dei Lincei (Rome) (1924); Membre Correspondant (later Membre Associé) de la Société de Pathologie Exotique (Paris) (1927), de la Société Belge de Médecine Tropicale (1922), de la Société de Biologie (Paris) (1927); Honorary Member of the American Society of Tropical Medicine (1938), Member of History of Science Society (U.S.A.) (1937), Foreign Corresponding Member of the Royal Flemish Academy of Medicine (Brussels) (1940); Member of the Anglo-Batavian Society (1925), of the Genetical Society (since its foundation), of the Society for General Microbiology (Foundation), of British Society for the History of Science (Foundation), of the Russian Protistological Society (1924).

During his career Dobell served on a number of committees. He was at various times a Member of the War Office Dysentery Committee; Adviser to the Ministry of Pensions; Member of the Foot-and-Mouth Committee of the Ministry of Agriculture, of the Pathological Methods Committee of the Medical Research Council; and a member of the editorial panel of *Parasitology*; at the Royal Society he was a Member of the Tropical Diseases Committee, of the Zoology Committee, of the Library Committee, and of the Tercentenary Sub-Committee.

Though primarily dedicated to science, Dobell's interests spread over a far wider field—in literature, philosophy and art. Among his favourite writers in his undergraduate years were Edgar Allan Poe, E. M. Forster, Oscar Wilde, Bernard Shaw, Guy de Maupassant and Ibsen. His philosophy was pessimistic, and at one time he was fond of reading Schopenhauer and Leopardi. As we have shown, the visual arts attracted him from his early youth. He could draw, paint and model skilfully, and this gift found much of its expression in the exquisite illustrations to his scientific papers. As a young man he gave rein to his imagination in strange, beautifully executed pen-and-ink drawings of symbolic character. These show the influence of Aubrey Beardsley and Max Beerbohm and of his beloved Japanese artists, but they also have something highly individual. They were inspired sometimes by a passing mood, but more often by other men's poems, by ballets he had seen, or, and most frequently, by the music of Chopin. All through life music was to him a vital necessity: but, as with his human sympathies generally, his taste was very personal, and his interest was concentrated on just those compositions that accorded with his own emotional nature. His feeling was all for the Romantics, and his idol was Chopin, whom he regarded as the greatest composer of all time.

The relegation of Art to the secondary position of a recreation was not achieved without a struggle that lasted all his life and had a profound influence upon his character, perhaps providing a clue to a complex nature understood by few. The inner conflict between the elements of Dobell's dual nature was the cause of much melancholy and unhappiness. He was by choice—and by wise choice—a man of science, and so committed to the line of cool detachment and objective analysis. But he had 'the artistic temperament . . . with all its excesses and defects', and therefore had always to reckon with the sub-

jectivity of the artist's outlook. He recognized this clearly, when in one letter (4/10/34) he referred to 'The everlasting conflict between the artistic impulse and the scientific mood, which are the elements in my composition', and in another (31/1/37) said: 'I always realize the difference between feelings and thoughts, and I am always able—through long practice—to control my actions, even if I can't yet master my emotions'. Even as artists go, he was exceptionally temperamental, and subject to violent emotional fluctuations—of enthusiasm or distaste for his work, or indeed for life itself. Time and again he wrote of 'this fearful depression of mind' that would overtake him without warning or seeming cause, and of 'acute pessimism, if there is such a malady'. The words 'long practice' quoted above, are significant, for much of this distress sprang from a subconscious revolt against the unrelenting severity of his self-discipline. For he was determined to work by 'dry light' alone. 'Dry light' is Bacon's *siccum lumen*, by which, according to Dobell, 'he meant the light of the intellect uncontaminated—or not wetted—by the emotions or will'. To live as well as to work by a dry light may in the end be searing, and it is fortunate that his emotions were not entirely under his control in the world outside the laboratory.

Other sources of Dobell's melancholy mood were the anxiety and frustration arising from an inadequate income, on the one hand, and his early upbringing on the other. A man of expensive tastes, he could seldom afford to indulge them, and very early he found himself financially responsible for his father's debts and for the support of his mother—who lived to be ninety. The extreme shortness of money in his youth cut him off from much that he saw his contemporaries enjoying and knew that he could appreciate better than they. And, when at Cambridge, the consciousness that even what he had to spend was borrowed money was bitter experience for one so proud. 'The ideal condition for work', he wrote in 1932, '—a large income, fixed and absolutely secure, and no family of any kind—are, I am afraid, unattainable in this life. I have always done my best to avoid entanglements with other people's lives, and to earn enough for myself: but the longer one lives, the more one seems to get involved in the general tangle of humanity, and the less possible does financial independence appear to be! When I think of the complications in all our lives, I often wonder how it is that I—or anybody else—ever manages to get any work done at all!' Yet five years later (1937) he cheerfully accepted the 'complication' of marriage. His wife was Monica, daughter of Alfred Augustus Baker, and step-daughter of the late Professor William Bulloch, F.R.S., one of Dobell's best friends. The marriage proved a very happy one, and for the last twelve years of his life he enjoyed understanding and support such as he had never known from another human being.

Probably a contributory cause of melancholy came from the memory of a difficult childhood, which had left a feeling that the whole world was against him and produced the offensive-defensive reaction characteristic of Dobell in his relations with most people. He had a reputation of being intellectually arrogant and difficult to get on with; and people resented his caustic tongue and

outspoken criticism. However, the first unfavourable impression was soon dispelled when one got to know him more closely, and recognized his high standards and sterling honesty in scientific matters, and his warm humanity in personal relations. It is true that, seeking perfection, he created about him an atmosphere of aloofness, for 'he never hunted with the pack, nor barked, growled, yelped with them either!' (Dobell 1949). Though he had no patience with stupidity or intellectual pretentiousness, he encouraged every honest effort, and to those who sought his advice he readily gave every assistance in his power. He had a great number of correspondents in all parts of the world, to whom he usually wrote after midnight, answering questions on nomenclature, giving advice on technique, referring them to little-known papers, and generally spreading himself in helpfulness to all inquirers who showed themselves sincere in the pursuit of truth. His memory for all that bore, even indirectly, upon his work and interests was prodigious, for he could put his finger at once upon any data required by some questioner: he had no need to 'look up the reference', since it was safely stored in his brain, and he wrote *currente calamo*, page after page, without erasure, in the graceful, legible, sensitive script that remained virtually unchanged to the last. He always had great sympathy for people in difficulties, and was sometimes recklessly generous in what he did to help younger men whom he liked or whose work he appreciated. He did not possess many intimate friends, for he was too limited in his choice and sympathies, but to those who belonged to his inner circle he was deeply devoted, and there was no trouble he would not take to advance a friend's interests. Among his friends were a few whom he worshipped as heroes: these were 'Elephants'—men like Adam Sedgwick, David Bruce, William Bulloch and D'Arcy Thompson. The rest were his peers or just fellow-'animalcules'.

We have already mentioned that in his religious outlook he was an agnostic, but he was never an out-and-out materialist, for he was certain that life could not be explained solely in terms of chemistry and physics, and was more ready than are most men of science to believe that certain regions of awareness may be accessible only to intuition: where the intellect seemed inadequate, he would admit that there may be other ways of 'knowing' than through the senses.

And, for all the fight he put up to direct his life along a chosen path, there was more than a streak of fatalism in him too. His letters constantly refer to: 'The feeling I get acutely from time to time that only the inevitable happens. . . . It is a queer feeling—doing things because you are impelled to do them by some unknown force; or rather (as this describes it more closely) arriving at predestined events, which have already taken place' (17/10/36). And again: 'I know Fate when I see her, and she has no terrors for me; so I accept the inevitable when it comes along' (31/1/37). Generally he ascribed this sort of thing to irrationality, but in 1939 he wrote to one of us: 'I am a complete fatalist—as all true men of Science must be'.

Clifford Dobell died as he lived, 'in harness', stricken suddenly in the midst of his work. A man of generous habits, fastidious tastes and wide cultural interests, he was an intellectual aristocrat and an erudite scholar. With him

passed away a *savant* who belonged spiritually to an earlier epoch, when 'Science walked hand-in-hand with Art' and not 'arm-in-arm with Trade'.

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Cecil A. Hoare
Doris L. Mackinnon

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